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ON THE GERM THEORY OF DISEASE.

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ON the surface and in the midst of a fluid containing organic matter undergoing decomposition are seen, with a powerful microscope, swarms of living particles, first called, by Mulder, monads. Some of these resemble minute granules, with Brownian movement, while the greater part look like tiny infusoria, and have an active motion of their own. Multitudes of short, staff-like bodies are present, and if the liquid shall have stood some time in a warm room, freely exposed to the air, little living things of a variety of shapes will have made their appearance. Prof. Cohn, in common with many others, arranges all these living particles under the genus Bacterium. Bechamp calls them microzymes. It is not necessary that the putrefying fluid shall be exposed at all to the air. You may tightly cork a perfectly fresh organic infusion, that to-day contains no infusoria, or even germs that are to be seen, and in a few days you will have the liquid swarming with living things.

Whence come they—the "infinitely little"? Are they animal or vegetable? This is a question on which there is a number of opinions. We have, first of all, the theory of heterogenesis, represented by Bastian, Huizanga, and Bennett.* The monads are the direct products of the transformation of the organic elements themselves. The granules of the living organism become the "microzymes" of decay and decomposition. The anatomical units of health become the bacteria of putrefaction. The most ardent panspermist now recognizes similar changes as a datum of science. Nobody, at the present day, expects to find a vegetable origin for cancer and tubercle. The anatomical elements of these diseases are now known to be derivable from the tissue elements of the part affected.† Moreover, all are agreed that the blood bioplasts of health—those white corpuscles of whose function we knew so little in our earlier student days—become the pus corpuscles of inflammation; these are

* See Edinburgh Medical Journal, March, 1868, for Bennett's views of the origin of Infusoria. He believes himself to have had experimental proof that all infusoria, vegetable or animal, in fermenting or putrefying liquids, originate in oleo-albuminous molecules which are formed in the fluids, and which, floating on the surface, form a proligerous pellicle. These molecules, by their coalescence, produce the lower forms of vegetable and animal life. He says he has observed their conversion into bacteria and vibriones.

† Virchow's Cellular Pathology, Lecture xix.

changes no more easily understood than the conversion of the microzymes of health into the bacteria of disintegration.*

I may remark that, to the evolutionist, who believes that vital motion is simply molecular motion, this view presents no inherent difficulty. If vitality be only a condition of certain of the allotropic states of protoplasm, then it is quite conceivable that organic products, undergoing retrograde metamorphosis, might break up into lower compounds, some of which would manifest life in its lowest phases.†

The second view is that of the "panspermists." All the infusorial animalcules seen in putrefying or fermenting liquids are the product of *germs* (ova or spores), introduced into the organism from without. The air is the receptacle of these germs, and it is from the air that we get them. If it be objected that living things make their appearance in organic infusions that have been tightly sealed from the air, it is replied that these germs pre-existed, dormant, in the organic material, waiting to become developed when the conditions should be favorable. If it be objected that if these germs pre-existed there they ought to be discernible, the reply is that practised microscopists do often detect them, stowed away in convenient recesses in the tissues or lurking in the fluids, and that if they do not find them it is no proof that they are not there, in an embryonic condition, so infinitesimal that no microscope has ever discovered or ever will discover them. If there should still be manifested ought of incredulity, we are pointed to the extraordinary fertility of the lowest vegetable organisms, whose reproductive activity is sufficient to fill with spores the earth and seas and the high heavens.‡ These spores we may draw in with every breath, we may swallow with every draught, and "the higher life is everywhere interpenetrated by the lowest life."§

The popular view respecting the living particles seen in decomposing fluids is that they are *fungus germs*. These little animalcules, if you will give them a chance, will develop into *moulds*. Hallier, for instance, has given us a beautiful theory if it should ever be proved true. The microzymes of decomposing fluids he calls "micrococcus." "Micrococcus" is simply spore dust from certain moulds, and is regarded

* The above theory certainly commends itself for its simplicity. According to Bechamp, the body is made up of these little plastide particles or microzymes. When they act harmoniously, there is health, and the fermentative processes go on regularly, but when they act inharmoniously the fermentative processes are deranged, and there is disease. The microzymes, he says, are not ferments themselves, but they secrete the ferment, a special ferment being necessary to the performance of each physiological function; they moreover produce bacteria, which latter are capable of returning to the microzyme state. After death, all organic matter returns to its original elements, and the microzymes are there to carry on the work of putrefaction.

† I am half persuaded that the views of the heterogenesists are destined ere long to prevail, yet in the present state of science it does not seem to me that we are warranted in accepting spontaneous generation as a datum of science. That living matter may, under suitable conditions, appear *de novo*, seems at least very probable, and that some, perhaps the larger part, of the bacteria seen in decomposing organic fluids may be heterogenetic transformations, carries a greater *prima facie* evidence of probability than the negative. This view could be maintained in entire consistence with the more popular view that the higher life is pervaded by the lower life, the microzymes of the highest and the lowest organisms being indistinguishable by the microscope.

‡ "That fungi should spring up everywhere, under fitting conditions, is readily explained by the enormous quantity of fruit which they produce. The dunghill Peziza sending its sporidia from its hymenium in a steam-like cloud may convince us of the powers of transmission which they possess, and a number of equally cogent examples might be adduced. A single Lycoperdon giganteum alone produces myriads of seeds."—Berkeley's Cryptogamic Botany, page 258.

§ Beale. Disease Germs, Part I, page 64.

as the principal agent in putrefaction. If the micrococcus comes in contact with substances that are capable of undergoing the alcoholic fermentation, its aspect changes, and it now appears under the familiar form of "torula," called by Hallier "cryptococcus." Sufficiently exposed to the air, *micrococcus* forms filaments of bead form, and is known as *bacteria*. Finally, sown on moist animal or vegetable substances, it developes into various aërophytic or anaërophytic molds. This theory, specious as it is, lacks sufficient confirmation. It is enough to refer to the recent investigations of M. Baudouin* to show that results negativing those of Prof. Hallier have been obtained by accomplished experimenters. Thus Baudouin, while failing to find any of Hallier's specific fungi in the cultures of contagious matter, affirms that bacteria are not fungus germs, but a transition between algæ and fungi. Prof. Lister, of Edinburgh, has arrived at similar conclusions.† His experiments indicate a curious series of changes on the part of microzymes, in accordance with the media in which they are placed. In Pasteur's solution, bacteria were seen to grow as motionless algoid threads with nucleated segments. In turnip infusion and in urine, under suitable conditions, they were seen to assume the characters of torulæ, a transformation to which Hallier also testifies. More minute species, first seen in milk, developed in urine into spirillum, which, on being introduced into turnip infusion, grew into fungoid forms, which, on being introduced into urine, reproduced the moving spirillum; as time passed, growing a smaller and smaller progeny, till they lost their spiral shape, and returned to their original form of minute bacteria.‡

Another independent observer, Madame Johanna Luders, in an interesting monograph, affirms that she has repeatedly witnessed the formation of bacteria and vibriones from the protoplasmic contents of well-defined spores of various fungi, in putrefactive fluids.§

The practical results of all these investigations may be thus epitomized. The living particles seen in decomposing organic fluids, if not precisely such transitional fungus forms as Hallier defines them to be, are, nevertheless, cryptogamic germs having a very close relation to fungi.

Admitting the cryptogamic origin of these living monads, what is their relation to fermentative and putrefactive changes out of the living organism? What is their relation to the organism in health? What is their relation to the organism in disease?

I. The term *fermentation* is now generally used in a somewhat restricted sense to denote the decomposition of ternary organic substances, their descent to a lower chemical plane, with evolution of carbonic acid, but without the evolution of any gases of offensive odor. When azotized organic compounds of higher molecular and chemical complexity disintegrate, their dissolution being attended with evolution of gases of offensive odor, we speak of the change as putrefaction.

It may be perfectly legitimate to speak of all internal changes taking place in organic substances as *fermentations*. The term would

* Culture des Infusoires. Paris, 1870. (Published in Coze and Feltz's book on Infectious Diseases.)

† "On the Germ Theory of Putrefaction," &c. Nature, July 10–17, 1873.

‡ Loc. cit. Second Paper.

§ Rindfleisch's Pathological Histology. Am. Ed. 1872. Page 32.

then include such isomeric transformations as that of starch into grape sugar by the ptyaline of saliva, the formation of oil of bitter almonds from amygdaline, due to the action of emulsine, and in fact all physiological and pathological changes taking place in the blood owing to natural or morbid influences. It seems to me, however, that it will be conducive to clearness and precision if the term be used in the limited sense of our latest chemical text-books.*

There is a marked resemblance between fermentation and putrefaction in the multitude of living particles which are developed during those processes. That fermentation is the result of disturbances set up in certain organic solutions by torulæ, has been pretty generally conceded since Cagniard de la Tour first demonstrated that yeast was composed of a multitude of minute vegetal organisms. That the various fermentations cannot be excited without such organisms, is now almost universally admitted, explain as we may the manner of action of the torula.† Moreover, the experiments of Helmholtz, Schroeder and Dusch have proved that the same relation holds between putrefying substances and the microscopical organisms that are found in them.‡ "It is now settled," says Prof. Cohn, "that without bacteria no putrefaction, and without yeast fungi no fermentation, takes place; that this decomposition is effected only through the sustenance and living activity of these microscopic cells."§

II. The question what relation these microscopical beings sustain to health is quickly answered. All the higher organisms seem to be indifferent to them. With an atmosphere charged with cryptogamic germs, as well as other organic particles, whose presence in the ordinary air of respiration has been lately demonstrated by Prof. Tyndall, we yet live and enjoy health, in spite of the rust and blight, and mould and mildew we inhale in almost every breath. Are we not daily exposed to the attacks of fungi akin to those found by Hallier in his cultures, and seemingly quite as terrible, and do we not, with impunity, pass the ordeal? Hallier's parasites seem to be, mostly, varieties of ordinary fungi?‖ How do we reconcile this with their banefulness?

Furthermore, there is reason to believe that in all healthy tissues and fluids vegetable germs exist, which, under favoring conditions, form mycetal growths, and facilitate disintegration. Beale, who is one of the most trustworthy observers, assures us that he has seen in old epithelial cells from the mouth of healthy persons, and from other mucous surfaces, germs precisely like those figured bodies represented in cholera dejections and the blood of cattle plague, by the advocates of the germ theory.¶

In fact, if portions of the blood or secretions of a healthy person be exposed in culture apparatuses to filtered air, they will, in warm

* Much of the obscurity that invests recent foreign works on the Germ Theory is owing to this vague use of the word *ferment*.

† See this subject, admirably treated by Huxley, in the Contemporary Review for August, 1872. Article "Yeast."

‡ Huxley's Lay Sermons, p. 360.

§ Nature, Jan. 2d, 1873.

‖ If we consider the great range of variability which characterizes the lower cryptogams, and that they vary according to the kind of soil on which they grow, and according to hygrometric and atmospheric conditions, we shall not be surprised to find a few common species giving rise to a multitude of different forms. This renders all attempts to classify these thallogens well nigh hopeless.

¶ Disease Germs. Part I. p. 64.

weather, be soon found swarming with "micrococcus," and if these putrefying fluids be "sown" on suitable soil *(a la mode Hallier)*, you will soon have an abundance of fungi, and most choice varieties.

Filiform cells and the interwoven tissue are almost the sole element of the fungi.* They derive their nourishment from the matrix on which they grow, and hence they are, as a rule, *epiphytes*. Yet their leading characteristic is that they feed on dead or decaying substances. It is true that there are exceptions to this:—that the Botrytis Bassiana produces that fatal disease muscardine in silk worms; that the Sporendenema Muscæ makes fatal ravages among the house flies at certain seasons; that numerous skin diseases in the human subject are due to vegetable parasites, as favus to the achorion Schœnleinii; Sycosis menti, Herpes circinnatus, and Herpes tonsurans to the Tricophyton; Porrigo decalvans to the Microsporon Audouini; Pityriasis versicolor to the Microsporon furfur; it is also true that the aphthous patches of Muguet are composed largely of the Oidium albicans, and that Leptothrix buccalis causes the teeth to decay.

It is not, however, proved that such fungi ever find favorable conditions for their growth and development, except where vital activity is low, where decay or disease is already present, or where filth, in the shape of retained excrement, furnishes just the proper soil. It is also true that the whole tribe of rusts and mildews do attack vigorous plants. That the Botrytis infestans causes the potato rust, is well known. The Oidium of the vine has ruined vine growers in almost every country where the vine is cultivated.† It has been shown that every species of corn has its fungoid parasites. It is easy to understand why animals, and especially the higher members of the animal kingdom, with vigorous circulation, and organs of secretion and excretion in full activity, should be so much less liable to disturbances from such epiphytal growths. One fact is determined by sufficient evidence. The inoculation of a healthy person with bacteria, or supposed fungus germs, is not necessarily dangerous. One may eat most of these moulds with impunity. The Kalmuck Tartars live on raw, putrid fish, or flesh of carrion, and they are said to be a healthy race.‡ The vital activity of the tissues is sufficient to preserve for a long time the fluids they contain from putrefactive changes. To quote from Golding Bird: "The blood in a vessel, even when its motion is prevented by ligature, does not *change*, in a space of time sufficient to convert it, if removed from the vessel, into a putrescent mass. The bile in the gall bladder, the urine in the kidneys and bladder, the fæces in the intestines, are examples of the same fact. This law even obtains in disease, for a serous or purulent effusion, the result of morbid action, will be preserved in the living cavities of the body unchanged, while a few hours would be sufficient to render it fœtid and putrid, if exposed out of the body to the influence of a similar heat."§

III. In view of the obscurity that still invests the metamorphoses of the lower cryptogams, it seems to me premature to predict aught respecting the causal connection of certain fungi, obtained from cultures of contagious disease matter, with the diseases in question. There is no proof that all that have as yet been found are not accom-

* Schleiden's Principles of Botany. Article, Fungi.
† Berkeley's Cryptogamic Botany, p. 261.
‡ See Bastian's Appendix E to "Beginnings of Life," vol. ii. p. cxxiv.
§ Golding Bird on Urinary Deposits. Second Am. Ed., p. 223.

paniments, or effects, and not causes of the diseased conditions with which they are found associated.* Hallier has not yet completed the cycle of proof necessary to establish the causal nexus between one single disease and the micrococcus found with that disease. He has relied exclusively on what logicians call the method of agreement—the method of difference he has not tried. It is of little account for him to show that the supposed cause A always exists with the disease B, and hence B is the effect of A. Into a preëxisting set of circumstances where B does not exist he must introduce A and produce the disease. This he has not attempted, and hence his speculations are of little worth.†

There is a wide field for experimentation in the lower animals, open to those who believe that the so-called zymotic diseases are induced by fungi. Every species must be studied in its physiological effects on the lower animals, and these experiments must, as far as possible, be repeated on the human subject. It will then be determined whether these microphytes have any action on the healthy animal system, except so far as they produce a toxic effect. It will be seen whether the blood of a living human being really furnishes such a favorable habitat for the growth and multiplication of the "living ferments" as Messrs. Coze and Feltz, with other partisans of the "animated pathology," seem to think.‡ That our knowledge of the fungi in their toxic and medicinal actions would be greatly extended by such a series of patient experiments is undoubted. Many of them are already known as powerful poisons, and some, as the secale cereale, have been utilized as medicines. The latter is no less notorious for the fatal gangrene it produces, when bread, made from the rye which contains the fungus in excess, is eaten. The fumes of the large puff-ball, Lycoperdon giganteum, have properties similar to those of chloroform,§ and the Agaricus muscarius, when dry, is a well-known promoter of intoxication.‖ All these effects, however, are strictly toxic.

Moreover, before the animated pathology can be established on a scientific basis, it must be shown, as Ranse has clearly pointed out, that the infected atmosphere contains spores identical with those of the fungi obtained by the culture of the bacteria, and that the same spores are in every way like the elementary corpuscles contained in the morbid products.¶ It will now be seen what a hiatus remains to be filled, before the animated pathology can be accepted as accounting for the origin of contagious diseases in general. We have now to ask, does this specious theory lay valid claim to any one zymotic disease?

* "As soon as fungi have developed themselves freely in animal fluids possessing special contagious properties, such as vaccine lymph and smallpox lymph, the specific characters of the poison become weak or disappear."—Beale's Disease Germs, part i. p. 82.

† It is, moreover, noteworthy that competent experimenters B udouin, Engel, Coze and Feltz have repeated Hallier's cultures, but without confirming Hallier's results. Beale, the accomplished English microscopist, has also found reason to reject Hallier's theories as utterly untenable.

‡ See Coze and Feltz. "Recherches Cliniques et Experimentales," &c., p. 16.

§ Berkeley's Cryptogamic Botany, p. 255.

‖ Prof. Schmiederberg, in investigating the philosophical action of this mushroom, last year, noticed that, when given to animals, it caused great dyspnœa, and "at the same time the arteries became empty, so that, when cut across, hardly a drop of blood issued from them." This latter is precisely the condition which exists in cholera. This favors the view that cholera is caused by a similar poison, and not by living cells, and shows the necessity of further investigation of the physical effects of fungi. See Medical Times and Gazette, October 11, 1873.

¶ Du Rôle des Microzoaires et des Microphytes, &c. Paris, 1870.

Huxley, in his celebrated address before the British Association, intimates his belief that the great problem will have to be solved for each zymotic disease separately. Here, then, is the place to speak of the indefatigable labors of our own countryman, Salisbury.

In the *American Journal of the Medical Sciences* for January, 1866, appears a somewhat remarkable paper, from the pen of Dr. Salisbury, who is professor in a medical college in Cleveland, Ohio. He claims to have made a microscopic examination of the expectorated matters, sweat and urine of patients in intermittent fever. He found an abundance of corpuscles, of an algoid type, resembling the lowest known vegetable organisms, the protococcus of Arctic snows; these he assigned to the group Palmellæ, giving them the generic name, *gemiasma* (meaning earth miasm). These palmelloid cells were found with wonderful constancy in the principal secretions of aguish patients, and corresponded exactly with cells obtained by suspending glass plates over broken ground at night, in places where malarious emanations were known to arise. He found the palmella encrusting the soil of aguish districts, and absent, both from the soil and the atmosphere, of regions known to be free from malaria. He determined the height to which the algoid corpuscles rise, giving a rational explanation of the experiential fact that regions above such level are free from ague. During the night, moreover, the air is more charged with these spores than during the day, as they are carried above the soil in the cold vapors, to fall to the ground during the sunshiny day, when the rarefied air can no longer hold them in suspension. This is his explanation of the fact that aguish districts are especially deleterious to night residents. The palmella is, according to Salisbury, the *materies morbi*, the *miasma vivum*. Not content with what the late Mr. Mill would style the method of agreement, he has endeavored to comply with the other necessary requirements of proof. He carried to non-aguish districts, from marshy ground, boxes of earth, covered with the unicellular vegetation, choosing hilly regions, free from malarious emanations, and on the sills of second-story chamber windows, exposed the boxes of pernicious earth. A plate of glass suspended over the boxes during the night was found covered with the palmella spores in the morning. Two young men, sleeping in the apartment, took the disease, the one on the twelfth, the other on the fourteenth day, no other members of the family being affected. In another similar experience, a young man and two children were exposed to the emanations from the palmella; the two children took the fever.* All this is very plausible, and it is to be desired that Salisbury's theory may be established by future observers, for then would the vexed question of malaria be settled. Salisbury's views are quite in accordance with those of some of the highest authorities. Thus Niemeyer (while ignoring Salisbury's experiments) has "no hesitation in saying, decidedly, that marsh miasm—malaria—must consist of low, vegetable organisms, whose development is chiefly due to the putrefaction of vegetable substances.†" Flint seems strongly disposed to accept Salisbury's discovery without reserve. He believes that something more than ordinary vegetable decomposition is necessary, since the disease is indige-

* For this statement of the contents of Dr. Salisbury's paper, I am indebted to Flint's Practice, 3d ed., and the pamphlet of Ranse, "Du Rôle des Microzoaires et Microphytes."

† Text-book of Practical Medicine, vol. ii. p. 621.

nous in certain localities, whereas there are other regions where vegetable decomposition occurs just as abundantly, that are free from malaria. Moreover, the poison is of a kind that is easily borne along by the wind, or arrested by trees and other barriers, or absorbed by bodies of water, which gives probability to the supposition that it consists of spores of some kind. Moreover, malarious fever is more common in the summer season, when, of course, cryptogamic vegetation is most abundant. Assuming, then, the cryptogamic character of miasma, it would seem to be an easy matter to verify or disprove Salisbury's results. As far as I have been able to ascertain, no microscopist of any note has verified them. Niemeyer, in the later, as in the earlier editions, of his text book, declares that no one has ever seen the spores whose existence he hypothecates as the malarial principle. Did the presence of the "Gemiasma" cells uniformly and universally attend intermittent fever, it is astonishing that they have escaped the observation of so many experts, the world over—men whose lives are devoted to the investigation of the "infinitely little," and who are even more familiar with the microscopic world than with the world of ordinary vision. This alone should inspire us with grave doubts concerning Salisbury's theory. Of foremost utility, at the present day, is the bold and original investigator; possibly the next position in the order of merit belongs to the conservative sceptic who doubts, and doubts till conviction is forced upon him!

"Truth never can be confirmed enough,
Though doubts did ever sleep."

Salisbury has left a great lacuna in his theory—he has not told us if he has found the palmella in the blood of his ague patients. This is a grave omission, for it is in the blood that the important changes take place. Moreover, granting the presence of the palmella in the urine of those who were the subjects of Salisbury's experiments, those corpuscles may have been only occasional concomitants, and not necessary etiological antecedents of the disease. Salisbury admits that he has found, associated with the palmella, other cryptogams, belonging to genera, Torula, Penicillium, Aspergillus, Sphærotheca, in the urine of ague patients; but these he regards as accidental. The palmella may have been equally accidental.

The strongest confirmatory evidence in support of the palmella theory is that afforded by the apparent communication of the disease by emanations from those boxes of earth. There is, however, even here, no absurdity in the supposition that the real miasma may have been a subtle chemical principle, possessing peculiar toxic properties, generated, under extraordinary conditions, from decaying organic matter, and intimately associated with the lowest algæ and fungi. The marsh miasm more resembles in its action a spreading chemical poison, than it does the "infinitely little" organisms, multiplying themselves in the blood, at the expense of the oxygen and pabulum of the tissues. Here is the startling fact that malaria does not reproduce itself in the system of the person affected—that, as Niemeyer expresses it, "there is no soil in the human body favorable to its development or increase." It thus resembles snake poison. The direct effects of malaria on the blood are those of a poison. "In the worst cases, the blood becomes speedily darker in color, and otherwise altered, and accumulates in extraordinary amount in the internal organs, where it then suffers still

further in consequence of its stagnation and want of purification by the ordinary processes of excretion. The fit of ague is the reaction of the vital powers against this cumulative influence of the poison on the blood; if the vital powers are strong, and the dose of the poison not overwhelming, the fit successfully removes the internal congestions, and partially restores the purity of the blood through increased excretion; but some poison being still in the system, similar effects are again produced, after longer or shorter intervals; and so alternate attacks and intermissions appear in succession."*

Till, then, more convincing experiments shall have been performed, the poison theory of the older pathologists will hold against the living ferment theory of the newer.

Salisbury, who certainly has the merit of being an indefatigable worker, has extended his investigations in other directions, and is so thoroughly possessed of the vegetable parasite hobby, that every communicable disease must be accounted thereby.

It is needless to say that Dr. Salisbury finds cryptogams in everything. In an interesting paper published in the *American Journal of the Medical Sciences* for January, 1868, he describes the Crypta syphilitica, and the Crypta gonorrhœa, two algoid productions, which he regards as the causes of venereal affections. It suffices to say that his observations have never been confirmed, and that his venereal cryptogams will have to rank along with the notorious Löstoffer corpuscles. Salisbury has published another paper, in which he traces that common exanthem, measles, to the presence and action of another microphyte, the so-called *alga morbilli*. This is of interest, considering that Hallier is equally certain that, commonest of all molds, the *mucor mucedo* is the real *materies morbi*.†

Messrs. Coze and Feltz have given us some most interesting researches, clinical and medical, on infectious diseases, in a work that has not been translated. Animated by an admirable scientific spirit, they have examined and analyzed the blood and secretions of a variety of contagious diseases, and have proved the presence of infusory animalcules and other figured bodies in abundance in those diseases. They have also demonstrated remarkable changes in the amounts of oxygen, carbonic acid and waste extractive matters in the blood, such as we should suppose would be effected by the swarms of microphytes that inhabit diseased organic fluids. It cannot but be that the work of decomposition and disintegration would be accelerated by the presence, multiplication and vital activities of such countless multitudes of living things, and this seems really to be their office in the economy of nature. Though not the principal factors, they are doubtless important adjuvants, of disease. It is where vitality is low, or absent, that they flourish. That all prostrating diseases, of whatever nature, should be attended with their presence in the blood and secretions, seems but a truism which every experienced microscopist is every day confirming. That they have any power to initiate disease in healthy organisms is yet unproved. Coze and Feltz, with all their painstaking, have not established, nor do they claim to have established, such nexus between their "mycrozymes" and any infectious diseases.

Prof. Ferdinand Cohn's little pamphlet, in German, on "Bacteria,"

* Quoted from Williams's Principles of Medicine, page 84.
† See his work on Parasitic Diseases, translated by H. C. Perkins.

which has been put into my hand by a friend, defends the specificity of the microzymes of all contagious diseases. The bacteria of contagious diseases, he says, are unlike those of ordinary fermentation and putrefaction. Prof. Cohn is not supported in these conclusions by the majority of his fellow-workers,* many of whom are as trustworthy observers as Prof. Cohn. Any differences between the microzymes would seem to be consequent on the altered nature of the fluids which are their habitat. Prof. Cohn feels certain that, in the cattle plague and other contagious diseases, bacteria are the "carriers and initiators" of the maladies. But it is evident that it is quite impossible to introduce bacteria into the blood of a healthy animal without at the same time introducing with them septic or putrescent matters which might initiate disastrous changes in the blood, and become the elements of contagion.

There are a few well-attested facts that of themselves seem to give the death-blow to the vegetable parasite theory of contagious febrile diseases. I will briefly enumerate two. The researches of M. Davaine in connection with that fearful epizootic disease in cattle known as *sang de rate*, which, communicated to the human subject, manifests itself in the form of malignant pustule, have determined that the epizootic is characterized by the presence of multitudes of little organisms, allied to vibriones in the blood of animals affected.† "Whilst this affection is always capable of being reproduced in a previously healthy animal by the inoculation of some of the fresh blood of an animal which has recently died of the disease, the blood of such an animal loses its powers whenever it becomes putrid. . . . Rabbits which had been fed upon the fresh organs of some of the animals that had recently died of the disease, almost invariably became affected, . . . whilst of other animals which had been made to swallow similar quantities of liver, after it had become fœtid (and, therefore, swarming with bacteria), only one out of eight died, and even that one, which was found to have suffered from an inflamed lung, did not reveal any trace of organisms in its blood." "These experiments," continues Pasteur, to whose work on the "Beginnings of Life" I am indebted for these facts, "seem only explicable on the assumption that in the cases where the 'blood' was communicated to other animals by inoculation, the disease was communicated, not so much by the direct multiplication of the stock of inoculated organisms, and then spread throughout the body, as because some of the inoculated matter had the power of setting up certain changes of a spreading character which soon sufficed to produce a condition of blood similar to that usually preceding the development of organisms in the disease.‡"

Another fact of importance, as telling against the germ theory, is recorded by Dr. Burdon Sanderson. "In sheep pox (a disease closely allied to, and even more virulently contagious than, smallpox) all the diseased parts are infecting, while no result follows from the inoculation, either of the blood or of any of the secretions; the liquid expressed from the pulmonary nodules has been found by M. Chauveau to be extremely virulent, certainly not less so than the juice obtained from the pustules." Dr. Bastian, in commenting on this remarkable

* See Beale's "Disease Germs," Part I. page 69, for support of this statement.
† Compt. Rend., 1864 and 1865. Quoted by Bastian in his "Beginnings of Life."
‡ Beginnings of Life, vol. ii. p. 363.

fact, observes that "although in other of these diseases the blood does undoubtedly exhibit infective properties, still the ascertained existence of even one exceptional case among maladies so contagious as sheep pox, seems absolutely irreconcileable with the truth of the germ theory, more especially when this theory was started principally to explain the phenomena of such highly contagious diseases."*

My criticism of the Germ Theory of Disease would be too incomplete were I to omit mention of another germ theory, for which we are indebted to that distinguished microscopist and histologist, Dr. Lionel S. Beale. Dr. Beale, while rejecting the vegetable parasite theory, believes in the *contagium vivum*, and gives us a new animated pathology. As this most modern doctrine of contagion is based on recent discoveries of the greatest value, I shall devote several pages to an examination of Dr. Beale's "Disease Germ" theories.

According to Beale, the active principle of all febrile contagious diseases is of *animal* origin. It is not a dead ferment, but living matter—bioplasm. It is bioplasm that has undergone degeneration, and is no longer fit to make tissue or assist function. There is a striking similarity, thus far, between this view and the older view put forth by Claude Bernard in the *Revue des deux Mondes*, and by Lucaze Duthiers in a pamphlet published in 1865. According to this, the animal organism is a colony of zoöids, or separate living units, forming a very close partnership, and working for their own and the common good. There may be riot and insubordination among the zoöids, and then there is disease, and the whole fabric is shaken. The refractory zoöids, like little pirates, attack certain organs, and the stress of the disease falls there. So Beale's little aggregations of degraded bioplasm assume a low, selfish, predatory mode of life; they no longer work for the common good, but to nourish and reproduce themselves. They display, in short, a low kind of reproductive activity; a tendency to produce generation after generation of bioplasts like themselves, but successively degenerating; living and flourishing under conditions where normal bioplasm would perish, and able to interrupt the social harmony of other organisms when introduced there, and generate there a riotous, wasteful progeny, at the expense of the pabulum, bioplasm or formed material which they there find. Dr. Beale claims that he has seen the bioplasts of vaccine lymph, of the cattle plague and of smallpox. In his book on Disease Germs, accurate delineations of these morbid figured bodies are given. So extraordinary is the vitality of these degenerate and diseased bioplasts that they will live weeks and even months after a partial desiccation. So minute is their size, that nothing smaller than a twenty-fifth or a sixteenth will bring them into view. He believes that the essential element of all contagion is of the same nature, but possessing various properties; just as bioplasm of one kind produces nerve, of another, epithelium, &c., so certain mysterious and unknowable conditions determine the production of a diseased bioplasm which produces malarious fever, variola, scarlatina, or typhus exanthemata, &c. Some kinds are more contagious than others, because finer and more subtle, and possessing greater vitality out of the system.

In favor of the peculiar views of Dr. Beale, it is proper to remark that his views in general respecting protoplasm are quite in accord-

* Op. cit., Appendix E, page cxxvi. Op. cit., p. cxxv.

ance with the teachings of the modern German school of biology. All recognize the white corpuscles as the chief blood bioplasts, possessing a wonderful constructive power in health and being the active agents in disease. The peculiar formative power which they possess was inherited at an early period from the bioplasts of the germinal area, and the capability of dividing and subdividing and giving rise to diversified structural and functional developments, is one of the mysteries which is not easily explained by any hypothesis of polarities or the analogy of crystallization.

According to Dr. Beale, the great bulk of our bodies is structure which these little toiling, amœba-like bioplasts have built up, and which has become "formed" or dead material, as little worthy of being called living as the shell of a mollusk or the corallum of an actinozoön. Although the doctrine might be open to the objection that, therefore, the work, functional and voluntary, performed by our organs and muscles is not *vital* at all, nevertheless it can be shown that even this is good modern scientific orthodoxy. It is an easy matter, in what are called "vital phenomena," to eliminate a host of factors which are now known to be physical or physico-chemical. Thus, Matteuci,* in his Lectures on the Physical Phenomena of Living Beings, has shown that the living body manifests the properties of endosmosis and exosmosis, capillarity and inbibition, just as do non-living substances; digestion and respiration are conducted in accordance with known physico-chemical principles; sanguification and the production of animal heat are strictly chemical processes, and even nerve conduction and muscular contraction are now comprehended under physico-chemical laws. The latter tissues manifest their properties, under stimuli, for some time after general somatic death has taken place.

The peculiarly vital phenomena of all organisms are those which are performed by the bioplasts—little structureless bodies, bearing a marked resemblance to the amœba, or Prof. Haeckel's deep-sea animalculæ;† these bioplasts exist scantily in the tissues, but abundantly in the blood and lymph. They initiate all active changes in health and disease. This is the only material capable of growth and multiplication. When supplied with just enough food, and in the normal state of things, this living material undergoes that regular series of changes which result in the construction and repair of tissue. When it lives faster than in health, by being supplied with an excess of pabulum, too rapid growth and multiplication take place; in other words, a morbid bioplasm results. In this way, pus is produced from bioplasts of every description.

The "bioplasm" germ theory is a very ingenious and plausible one, and may, perhaps, be deemed worthy of provisional acceptance, till a better is found; but it is as yet unproved. According to Dr. Beale's admission, all kinds of germinal matter look alike; they are undistinguishable by any characters which the microscope or chemistry can discover, and yet he believes that he has seen the diseased bioplasts of certain infectious diseases! That I have not misrepresented

* See his "Lectures on Living Beings," translated by Pereira. See, also, Maudsley's Body and Mind, p. 124 (foot note). Also, Schleiden's application of the same method to *plants*, in his Principles of Botany, Lankester's translation, p. 84.

† See Popular Science Monthly, for December, 1873.

Dr. Beale will be seen by reference to the following pages of his work on "Disease Germs," Part II., page 29; also page 162. I will here quote but one passage:—

"It will strike many as very remarkable that the highest magnifying powers hitherto placed at our disposal, serve but to convince us that a minute particle of the bioplasm of the most malignant tumor, or the most rapidly growing pus corpuscle, resembles, in every particular that we can ascertain by observation or experiment, a minute particle of healthy living bioplasm from the blood or from any tissue, and it is proved beyond a doubt . . . . that the living particles in vaccine lymph cannot be distinguished from those present in normal lymph and chyle."—Page 162.

This being true, how does Dr. Beale know that the particles of germinal matter which he finds in vaccine lymph, the lymph of cattle plague or cholera, are not *natural* to the fluid in which they are found; simply secondary products, as all inflammatory and febrile affections are characterized by an extraordinary increase of germinal matter in inflamed foci and in the blood; this being consequent on the specific irritation, of whatever kind, that excites the morbid outbreak? To claim that they are the germs of the disease, is begging the question. Our German pathological teachers have taught us to expect in all such cases a "proliferation of embryonic cells." Dr. Beale's supposed contagious bioplasts, then, may be simply *effects*, and not *causes*, of the morbid conditions with which they are found associated. It would be as difficult to isolate these bioplasts, for purposes of experiment, from the septic elements in which they are entangled, as Prof. Cohn's bacteria, which, in some inconceivable way, he supposes he has strained out of diseased fluids.

My object has been to review the prominent phases of the Germ Theory; to criticize popular theories, rather than propound any of my own. The scientific world is inebriated with speculation; the fogs and mists of error blind honest searchers after true knowledge, and the torchlight of induction shines dimly where clear light is needed. In rejecting the Germ Theory as untenable, we have either to confess our ignorance of the causes of all febrile and inflammatory contagious diseases (and it were better to rest content with ignorance than entertain beliefs that are not true), or, guided by analogy, to accept the alternative that the principle of contagion is a subtle chemical ferment, an organic poison, generated in the body of the diseased individual, derived from other diseased individuals, by infection through wounds, as in cow pox and hydrophobia; by infection by contact, as in gonorrhœa and syphilis; by infection by exhalations in the breath or other secretions, as in the case of measles, scarlet fever, whooping cough, and other infectious fevers.* Assuming the chemical theory of contagium, it will be readily comprehended that it might be as difficult to isolate and obtain for purposes of analysis and experimentation the septic or toxic material from the blood and secretions of the animal or person affected, as it would be to detect and identify morphia, strychnia, or any other powerful organic poison in the blood, secretions, or nervous tissues of a person poisoned by either of these drugs. Accepting the chemical theory, we can understand why the poison should be elaborated by preference in certain tissues, from which, as from certain

* See Williams's Principles of Medicine, p. 88.

local foci of contagion, the disease radiates over the system.* We can, moreover, understand why the specific poison "does not seem to be immediately reproduced in the blood of the person affected; rather a set of changes are set up in the blood, which ultimately lead to the evolution of such a poison in some part or parts of the body, so that, as Mr. Simon says, "bowels, skin, kidney, tonsils are the favorite resorts of the several fever poisons, just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated."

Dr. B. W. Richardson, in the *Medical Times and Gazette* for Nov. 5, 1870, advocates a similar view:† "A person suffering from a communicable disease is poisonous precisely as a *cobra di capello* is poisonous—that is to say, he is producing, by secretion, an organic poison, which, if it comes into contact, in the right way, with a healthy person, will reproduce disease."‡ As for the predisposing causes of these febrile and inflammatory communicable diseases, we may enumerate, first, a lowered vitality from heat, fatigue, intemperance, or other debilitating influences; second, certain not well understood conditions of the atmosphere, as a plus or minus of ozone or electricity, or the presence of noxious gases; third, as pointed out by Dr. Carpenter,§ "the presence in the system of an azotized matter, tending to decomposition, whether arising from a rapid disintegration of the materials of the body, as occurs after child-birth, accidental injuries, surgical operations, excessive fatigue, or extreme privation—or caused by impaired action of the depurating organs of respiration and excretion, or directly introduced in the form of unwholesome food in a decaying state."

CONSUMPTION OF HORSE-FLESH IN PARIS.—The horse-butchers, during the first quarter of 1874, have sold 2,111 horses, mules and asses for food. In 1872, the numbers were 1,275, and in 1870, 980. The same progress is making in the Provinces. The Society for the Propagation of the Sale of Horse-flesh has just decreed a medal to M. Carder, for his mode of preserving horse-flesh. Some of this, which was prepared by him in February, 1871, and examined in April, 1874, was found to have presented every analogy to beef preserved by the best methods.—*L'Union Medicale; Medical Times and Gazette.*

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* Instances are abundant which show that the natural secretions may become poisonous through peculiar depressing influences, as disturbing emotions. The maternal milk has been known to assume noxious and even deadly properties, owing to the power of fear on the lacteal secretion. The bite of infuriated animals, not rabid, has developed symptoms similar to those of rabies.

See this question discussed fully by Dr. Carpenter, in his late work "On the Principles of Mental Physiology."

The opinion above expressed finds an advocate in Youatt. In his valuable work "On the Dog," p. 221, he compares the action of the hydrophobia poison to that of infectious fevers. He says of the rabid virus:—"It has never been analyzed, and it would be a difficult matter to analyze it. . . . . It must be received into a wound. It must come in contact with some tissue or nervous fibre, and lie dormant there for a considerable but uncertain period. . . . . It lies for a time absolutely dormant, ... but at length the tissue on which it has lain begins to render it somewhat sensible and assimilates to itself certain elements. The cicatrix begins to be painful, and inflammation spreads around. The absorbents are called into more powerful action; they begin to attack the virus itself, and a portion of it is taken up and carried into the circulation, and acquires the property of assimilating other secretions to its own nature, or it is determined to one of the secretions only; it alters the character of that secretion, envenoms it, and gives it the power of propagating the disease."

How much like this, the action of the vaccine virus, variola, and other contagious febrile poisons.

† Bastian's Beginnings of Life, vol. ii., Appendix E.

‡ Quoted by Bastian.

§ British and Foreign Medico-Chirurgical Review, January, 1853.

Progress in Medicine.

REPORT ON DISEASES OF THE NERVOUS SYSTEM.

By James J. Putnam, M.D.

Pathology of the Cortex Cerebri.

Since the publication of the highly important experiments upon the localization of centres for muscular movements in the cortex cerebri, made first by Fritsch and Hitzig, of Berlin, afterwards with, in the main, similar results, by Nothnagel, of Berlin, Ferrier, of London, and others (vide this Journal for July 24 and 30, 1873, and April 30, 1874), a number of clinical cases have been reported, in which defined lesions of the cortex produced muscular convulsions, with subsequent partial paralysis, so localized as to strikingly confirm the justice of applying to man, in principle, at least, the results obtained with the brains of animals (*Archiv für Psychiatrie u. Nervenkr.* 1872, Bd. iii. case by Hitzig; *Virchow's Archiv*, Bd. lvi., case by Wernher; *Med. Times and Gaz.*, Nov. 30, 1872, cases by Hughlings Jackson; also *Arch. für Psychiatrie*, Bd. iv., 3 cases by Bernhardt). Among these should be classed a case reported by Bartholow (*Am. Journal of the Med. Sci.*, April, 1874), who, having a patient in whom the dura mater over the upper surface of the brain had been extensively laid bare by an epithelioma of the bone, thrust needles to different depths into the brain, and passed a current of electricity between them, with the effect of causing contraction of the muscles of the neck, with the extensors of the arm and leg, both on the opposite side of the body. An epileptiform attack, limited to the arm, shoulder and neck of the opposite side, occurred, also, after an injury of some severity to a limited region of the cortex.

These experiments and cases, taken in connection with the large number of instances where aphasia has been associated with disease of the convolutions about the fissure of Sylvius,* together with the presumptive evidence furnished by anatomy, would seem unquestionably to teach that the different muscles of the body are represented in certain regions of the cortex cerebri, constituting centres, from the irritation of which the corresponding muscles can be excited to contraction, although, to judge from pathological observation, the arrangement of these cerebral centres may not be, for every individual, precisely the same.

This view has, however, been combatted, especially by Dr. Brown-Séquard,† as has been already stated in this Journal (*loc. cit.*), who brings forward a large number of cases to prove that disease may exist in any part of the brain without causing symptoms at all, or only on the same side of the body with the lesion, and conversely, that disease in almost any part of the brain may produce almost any symptom. From these, and other similar facts, he concludes that the theory which assigns special functions to the different centres in the brain, especially the cortex cerebri, and explains symptoms by the direct irrita-

* For summaries of these cases, vide papers by Lohmeyer, in the Arch. für Klin. Chirurgie, and Menzel, in the Wiener Med. Wochenschr., 1874.

† Vide, among other papers, the Archives of Scientific and Practical Medicine for January, February and March, 1873.

tion or destruction of those centres, is absolutely untenable, and proposes the theory instead, that the apparent lesion acts *indirectly* upon "distant centres," exerting over them an irritating or a paralyzing (inhibitory) influence, just as peripheral lesions are known sometimes to do. In this way, alone, he believes, are symptoms of disease in the cerebral lobes, the cerebellum, and its peduncles produced, and, in part, those of disease of the base of the brain.

Though never to the same extent, this indirect influence of cerebral lesions has already been recognized by many observers,* but it is also widely questioned whether the principle of the possible assumption by one part of the brain of the functions of another part which may have been destroyed, cannot be so extended as to explain many of the anomalous cases referred to, leaving the theory of the *direct* influence of lesions, for which the positive evidence is so strong, otherwise intact.

This principle is well recognized in general pathology, and, in view of the intimate connections existing between different parts of the ganglionic masses of the brain, particularly the cortex cerebri, its influence in cerebral pathology might fairly be looked for, especially if we admit that the nerve-cells, of the cortex for example, are less distinguished from one another by any physiological peculiarities than by their education, i. e., by the excitations which they habitually receive and transmit.

There appears, moreover, to be the objection to Dr. Brown-Séquard's argument, that, while specifically disproving the existence of "centres," even at the base of the brain, he really presupposes their existence somewhere, as, indeed, the occurrence of definite symptoms, such as we observe clinically, whether produced *directly* or *indirectly*, demands, and it seems to be necessary in either case to admit the validity of the principle of vicarious functions in one form or another.

The positive evidence for the existence of centres, in the modified sense, as regards the cortex cerebri, has already been alluded to.

Dr. Hughlings Jackson, whose observations have several times been mentioned in these Reports *(loc. cit.)*, insists on the importance of studying them rather through the careful observation of unilateral convulsions beginning in special muscles, or muscular groups, than by the class of paralyses, for the reason stated, that from absence of paralysis we cannot confidently infer the presence of corresponding centres in the cortex, while from convulsion we can infer their presence and their excitation.

According to his, and others', view of cerebral physiology, we can already say, in general terms, that the whole muscular system of the body is probably represented several times within the central nervous system, in different masses of gray matter, that of the spinal cord, of the cerebral ganglia (except, perhaps, the optic thalamus), of the cerebellum, and of the cortex cerebri, from any one of which it is capable of being excited, entirely or in part, to functional activity. Among these ganglionic masses, it is the cortex cerebri which is so placed as to receive the greatest number and variety of nervous impressions through the channels of the senses, which it also has the power of storing up for future use ; and there also the greatest variety and number of voluntary impulses naturally have their origin, the active vol-

* Vide, for example, Jaccoud, Path. Interne, 4th ed., i. p. 286.

untary impulse (i. e. the nervous impression which excites the nerve-centres in which it has its seat so strongly that through them muscular action is called out) being the resultant of all the impulses, or impressions, present at any given moment in the brain.

The exclusive employment, then, of single muscles, or of a number of muscles in new, or comparatively new, combinations for definite purposes, is, according to this view, a function (speaking physiologically) of the cortex cerebri.

Less highly differentiated, or complicated, i. e., more automatic, movements or even complicated ones, if they have become familiar through repetition, may be called out through the influence of the cerebral ganglia, the cerebellum and the spinal cord. The experiments of Hitzig and Ferrier plainly afford striking support to these views.*

Hitzig, and still more Ferrier, found, moreover, that when the irritation of a definite point of the cortex cerebri, with induction currents, was continued for a certain time, epileptiform convulsions of the muscles on the opposite side of the body could be induced, beginning in the muscles corresponding to the point irritated. It was found also, by both Hitzig and Ferrier, that when any motor centre was cut out, the animals were not paralyzed for the more or less automatic movements of walking or running, but that they would allow the corresponding member to be placed in awkward positions without attempting to correct them, indicating an impairment of muscular sense, although general sensation seemed unimpaired.

Applying these views to pathological cases, we can, according to Hughlings Jackson, assume that localized unilateral convulsions, or convulsions beginning as such, even if they spread afterwards to other parts of the body, are due to irritations *centring* at definite points of the cortex cerebri.†

Such convulsions occur consequent on the nutritive changes due to embolic plugging of arteries, to syphilitic and other tumors, or to localized meningitis, if they cause irritation of the cortex cerebri at certain points (vide *Medical Times and Gazette*, 1873 and 1874, papers by Jackson and Broadbent), and Jackson considers it probable that many cases now classed under the head of epilepsy (a name which he thinks should not be used to designate a collection of symptoms, but a condition of mal-nutrition of nervous centres, which renders them liable to periodic outbreaks of excitement, and which may affect many different parts of the brain) are due to lesions there, although these lesions may easily escape notice, causing, as they do, irritation only, not destruction, of the parts involved. Convulsions of this kind may be followed by more or less temporary, incomplete paralysis, or loss of acquired dexterity, of the parts attacked, of which temporary aphasia is a type. (Speech, unless simply ejaculatory, occurs when the excitations, of which the subjective side is thought, excite the motor centres for the organs of articulation, &c., so strongly that muscular contraction results, and in the conception of words *without* their utter-

* The criticism made by Brown-Séquard and others that, in these experiments, it is the irritation, not of the cortex, but of ganglia lying beneath it, which causes the movements, is discussed in this JOURNAL for July 16, 1874.

† Compare Reports on Mental Diseases in this JOURNAL for July 31, 1873. Also, West Riding Asylum Reports, article Epilepsy. Hughlings Jackson believes, on theoretical grounds, that the spreading takes place through the indirect influence of the arterial districts.

ance, the same centres are implicated, as Hughlings Jackson believes, but less strongly excited.)

Such temporary paralysis may, perhaps, be due to exhaustion of the nervous centres, consequent on their over-action.

When the paralysis is more extensive and complete, as in most cases of cerebral hemiplegia from hæmorrhage, the corpus striatum, or motor tract connected with it, is generally the seat of the lesion, and the paralysis is not necessarily preceded by convulsion, as is almost invariably the case when the cortex is affected.

Shock and Cerebral Concussion.

Our knowledge of these somewhat obscure affections has received some valuable additions at the hands of the experimental physiologist, within the past few years, as is clearly shown in some excellent papers by Prof. Fischer, of Breslau,* and Brunton, of London.†

The characteristic *symptoms* of *shock* are stated as:—pallor and coldness of the skin, weakness and irregularity of the pulse, oppressed and sighing character of the respiration, nausea and vomiting, and apathy of mind without entire loss of consciousness, in brief, a cholera-like prostration; its *causes* as: painful impressions on the sensitive nerves (as in large burns), injuries to bones, and, *above all, injuries to the abdominal viscera* and the genitals.

Besides this form there is another, called by Travers "prostration with excitement," of which we will only say here that, according to Fischer, it is generally secondary to the form described.

Shock has been pathologically defined by Savory as "the paralyzing influence of a sudden and violent injury to nerves on the activity of the heart." According to Brunton, this probably occurs in many cases at the outset at least, but the important agency, to which especially the continuance of the symptoms is due, is believed to be *reflex paralysis of the vascular system of the whole body, but particularly of the abdominal cavity*, due to the inhibitory influence which the injury exerts upon the vaso-motor centres, which lie mainly in the medulla oblongata.

That such a reflex paralysis is physiologically possible is an acknowledged fact, and the experiments of v. Bezold and Bever, and others, have shown also that the vascular, especially the venous, system of the abdomen, supplied by the splanchnic nerves, may dilate, when those nerves are cut, so much as to contain nearly all the blood of the body, which stagnates there as in a reservoir. It is especially upon the experiments of Goltz, now of Strasburg, however, that these pathological views are based, who found that a number of light, successive blows upon the abdomen of a frog‡ caused a temporary standstill of the heart; and, further, that after it had begun again to beat, it received and sent out but a small quantity of blood, indeed none at all if the frog was held upright, while examination showed that the missing blood had collected, as described, in the veins of the abdomen.

Goltz at first believed that the paralysis affected solely the vessels of the irritated part, the abdomen, but it was found to be universal, and producible, moreover, by a jar of the whole body.

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* Volkmann's Sammlung Klinische Vorträge, Nos. 10 and 27.

† Practitioner, October, 1873.

‡ Brown-Séquard reports that he had made similar observations upon other animals still earlier.—Reporter.

Through this rapid abstraction of blood, and consequent failure of the circulation, it is claimed that the pallor of the skin, rapid fall of temperature of the body and cerebral symptoms are readily explained. When recovery takes place, it is rather gradual, since the veins contract again but slowly, more slowly than the small arteries; and this, as Brunton believes, gives us a suggestion as to the pathology of *fainting* (syncope), of which the symptoms strongly resemble those of shock.

In the treatment of shock, there are two therapeutical indications: to cause the vessels to contract, and to stimulate the heart's action, besides, of course, keeping up the temperature. Stimulants, ammonia held to the nose, excitation of the skin, are all agents recognized experimentally and clinically, as useful for these ends, and Brunton recommends further, on theoretical grounds, the giving of digitalis in large doses, as has, he says, indeed, been done by Wilks* (half a drachm of the tincture every hour for seven doses), with excellent results, in one case where other means had failed.

The pathology of *cerebral concussion* is believed by Fischer, reasoning by exclusion and analogy, and from the results of autopsies, to be analogous to that of shock, except that the reflex paralysis (in an uncomplicated case, which is a rarity) affects the vessels of the brain alone. He points out that dilatation of the vessels so interferes with the circulation that the tissues are no longer properly nourished, and are virtually in a state of anæmia, which accounts for the coma, and, acting on the heart through the vagus nerve (for whose nucleus in the medulla oblongata the absence of well-oxydized blood is known to constitute an irritant), also for the slow, irregular pulse—the two characteristic symptoms of that state. Shock and concussion are often followed by symptoms which belong, in general, rather to the direct injuries to the central organs or their membranes which so often occur, but there is some reason to believe that the periodic outbreak of nervous symptoms which are often observed in the sequel (such as epilepsy), may sometimes be the indication of a morbid habit which was instituted in the original attack.

At a recent meeting of the Obstetrical Society of Edinburgh, Dr. Cuthbert showed a rare and interesting case of strangulation in utero. The mother of several healthy children miscarried for the second time March 29, 1874. Her last menstruation was on Dec 17, 1873; consequently, the fœtus was about eight and a half months old. It was perfectly healthy and well developed. No reason could be assigned by the patient for the accident. On examining the fœtus, the funis was found coiled twice round the neck, and under the coiling it was tightly tied in a single knot. The part of the funis from the umbilicus to the neck was completely on the stretch, and its whole length from the insertion in the placenta to the umbilicus was about seventeen inches, or two and one half times the length of the fœtus. The part continuous with the fœtal circulation was round, and of a natural size; but the other part, viz., from the neck of the fœtus to the placenta, was small, and cord-like.

There can be no doubt that the death of the fœtus, and, consequently, the miscarriage, was caused by the tying and coiling of the funis round the neck. Probably, it was also hastened, as the fœtus grew larger, by putting the smaller part of the cord on the stretch, which would tend to tighten the knot and coils; at the same time it would also stop the circulation.—*Edinburgh Medical Journal*, June, 1874.

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* Medical Times and Gazette, Jan. 16, 1864.

Reports of Medical Societies.

American Otological Society.

The seventh annual meeting of the Society was opened at the Aquidneck House, Newport, R. I., on Wednesday, July 15, 1874, at 10.45 o'clock, A.M., the Vice President, Dr. C. J. Blake, in the chair. Thirteen members were present, representing New York, Brooklyn, Philadelphia, Boston, Rochester, Detroit, St. Louis and Pittsburg.

The chair appointed as the Executive Committee, Drs. H. D. Noyes, O. D. Pomeroy and John Green.

The Treasurer's report was read and was referred to Dr. Buck as the Auditing Committee, who reported it correct, and it was accepted.

The nomination of Dr. Ezra Dyer, of Pittsburg, was reported by the Executive Committee, and, on ballot, he was elected. The same committee reported the following bulletin of papers, which were read in order, and referred to the Publishing Committee.

1. Report on Otology. Drs. Burnett and Blake.
2. Case of Otitis Media, with Sinus opening into the Pharynx. Dr. A. Mathewson.
3. Aural Polypus having a Cartilaginous and Osseous Base. Dr. O. D. Pomeroy.
4. Ultimate Forms of Granulation Tissue in the Ear. Dr. A. H. Buck.
5. Perforations of the Membrane of Shrapnel. Dr. C. J. Blake.
6. Chronic Purulent Inflammation of the Tympanum, with Perforation of the Membrana Flaccida. Dr. C. H. Burnett.
7. Case of Irritation of the Chorda Tympani; Paralysis of Facial; Polypoid Growths on the Membrana Tympani. Dr. H. D. Noyes.
8. Case of Chronic Suppurative Inflammation of the Middle Ear, terminating fatally from extension of the Inflammation to the Middle Ear and the Brain; occurring in the practice of Dr. Agnew. Dr. David Webster.
9. Medicated Bougies. Dr. A. H. Buck.
10. Cases illustrating Tenotomy of the Tendon of the Tensor Tympani. Dr. O. D. Pomeroy.
11. The Mechanical Value of Weight in the Ossicula. Dr. C. J. Blake.
12. Some Neuralgias in and about the Ear. Dr. J. Orne Green.
13. A case of Ménière's Disease. Dr. C. H. Burnett.

The thanks of the Society were given to Drs. Burnett and Blake for the very satisfactory manner in which they had performed their duties as the Committee on Otology, and it was voted that one hundred extra copies of their report be furnished these gentlemen.

After the reading of the second paper, at 1.45 o'clock, the Society adjourned till 4 o'clock.

Second Session.—The Society was called to order at 4.15 o'clock, and the minutes of the first session were read and approved.

The nomination of Dr. C. S. Merrill, of Albany, was received from the committee, and, on ballot, he was elected.

The reading of the papers was then concluded.

Dr. Rider announced the death of Dr. J. W. Lawton, of Syracuse, and gave a short account of his life and last illness.

The Publishing Committee were directed to revise the constitution and by-laws, and submit their report at the next meeting.

The following officers, nominated by the Executive Committee, were elected for the ensuing year:—

President.—D. B. St. J. Roosa, M.D.

Vice President.—Clarence J. Blake, M.D.

Secretary.—J. Orne Green, M.D.

Committee on Otology.—Clarence J. Blake, M.D., A. Mathewson, M.D.

Publishing Committee.—J. Orne Green, M.D., J. S. Prout, M.D., H. G. Newton, M.D.

Bibliographical Notices.

Legal Responsibility in Old Age. By GEO. M. BEARD, M.D. New York, 1874. Pp. 42. 8vo.

THIS pamphlet is a reprint of a paper read last year before the Medico-Legal Society of New York. It is a research into the relation of age to work, and is not a treatise on senile dementia. The author states that he has noted the age at which great men have done the original work on which their fame rests, and has prepared a table as the result of his investigations. The "golden decade" he places between 30 and 40, from which time mental power declines. Seventy per cent. of the work of the world is done, he thinks, before 45. The average age of those embraced in this list is sixty-six. The quantity of work done by the aged is greater, but in quality and originality the advantage is on the side of youth.

The moral faculties are subject, he says to the same law, and decline in old age, from over-exercise of the lower at the expense of the higher nature through life; from diseases of the brain, or of the body affecting the brain, and because of the intellectual decline.

The idea that the mind declines later than the body has been generally accepted because of our natural reverence for the aged; because reputation is not exactly coincident with the work it rests on, but may even be posthumous; and from the false teaching of works of art which represent distinguished men generally in advanced life.

The paper concludes with some remarks on the frequency of crimes in the aged, on their occasional incapacity as testators, and on the best method of modifying the present system of conducting trials where insanity is in question.

There is a certain incoherence between the parts of this paper which suggests the adaptation of an old theme to the purposes of an address. This is no fault in itself, but when the title and the audience are considered, the want of vital connection between the literary, scientific and legal bearings of the subject are more apparent. The setting forth in the form of an essay the not entirely novel fact that the mental powers are most active between the ages of 30 and 45, the period known as the "prime of life," and supporting it by illustrations from history and biography, hardly warrants the writer's claim "*to the scientific discovery of the law of the Relation of Age to Work.*" He, in fact, admits that in his general conclusions he has been anticipated by many illustrious thinkers.

Granting the law, however discovered, the author does not very clearly show its bearing on the legal responsibility of the aged. The disproportionate decline of different faculties, the irregular decay of mind, which produces correspondingly limited responsibility and capacity, belongs to the consideration of senile dementia, which is an exceptional and not a general consideration. It is hardly to be expected that the Law, so slow to discover impaired responsibility in the dementia of old age, will take cognizance of nicer gradations, or provide a descending scale of responsibility for the closing decades of life.

This paper is suggestive, but Dr. Beard need not rest *his* claim for original work on so slight a basis. T. W. F.

BOOKS AND PAMPHLETS RECEIVED.

On the Torsion of Blood-vessels. By Robert McDonnell, M.D., F.R.S., one of the Surgeons of Dr. Stevens's Hospital, Dublin. Reprinted from Archives of Scientific and Practical Medicine. 1874.

On the Value of High Powers in the Diagnosis of Blood-Stains. By J. G. Richardson, M.D. Extracted from the American Journal of Medical Sciences.

Transactions of the Kentucky State Medical Society, 1874, Nineteenth Annual Session.

Census of the City of Providence, May 1, 1874. By Edwin M. Snow, M.D., Superintendent.

Annual Reports of the Trustees, Treasurer, &c., of the New Hampshire Asylum for the Insane to the New Hampshire Legislature, June Session, 1874.

Boston Medical and Surgical Journal.

Boston: Thursday, July 30, 1874.

The Third Annual Report of the Board of Health of the city of New York contains a most thorough and interesting history of the equine influenza, which invaded this country during the year 1872–3, originating in Toronto, and spreading rapidly throughout the United States, crossing the Mexican border to Central America, and transferred by means of steam transports to Cuba. The sub-division of this report relating to the origin and mode of progress of the epizoötic was entrusted to Dr. A. B. Judson, who has collected and put upon record certain important data, indicating the laws which appeared to govern its progress and mode of conveyance; and to this portion of the report we desire to call the especial attention of our readers. Dr. Judson arranges the conclusions to which, as the result of his investigations, he has been led, in the form of three propositions, which are illustrated by well executed maps. His first proposition is that "*epizoötic influenza does not spread by virtue of any of the recognized atmospheric conditions of cold, heat, humidity, season, climate or altitude.*"

The correctness of this proposition is demonstrated by the fact that the disease advanced with equal rapidity in the north, during the cold months of winter, and in Central America during the heat of July; in the dry atmosphere of the inland country, and the moisture of the seaboard; in the high plateaux of Mexico, and the low, swampy districts of Louisiana.

Second. "*Epizoötic influenza does not spread solely by virtue of unrecognized atmospheric conditions.*" This proposition is logically proved by tracing the progress of the disease upon maps, upon which are drawn arcs and radii, by reference to which it is shown that the disease appeared at points equally distant from Toronto at totally different dates, whereas it broke out simultaneously in different cities hundreds of miles distant from each other, all lying in the same direction from Toronto. Then, too, the irregularities in the breaking out of the malady at different localities are so numerous and striking that it would prove absolutely impossible to frame or even imagine any atmospheric law with which these abnormal appearances would not conflict, unless we are able to believe in the existence of unrecognized atmospheric conditions, whose phenomena are totally different from the phenomena of the recognized atmospheric conditions. Dr. Judson considers himself, therefore, justified in maintaining that while it is proved that the virus of this malady is *not* capable of being transferred

through the medium of the air for short distances, and over limited areas, on the contrary it *is* proved that the spread of the disease is not to be ascribed solely or chiefly to unrecognized atmospheric conditions. Furthermore, the immunity of certain localities could not be explained upon any atmospheric hypothesis.

The third proposition is, "*Epizoötic influenza spreads by means of its communicability.*" The proof of this proposition is also of a logical character, but is so abundant and forcible, as to be absolutely convincing. This proof is included under three heads, which may be briefly stated as follows:—

a. The epizoötic penetrated every place on this continent, which was in communication, by means of horses or mules, with localities in which the disease existed. So far as concerns Cuba, at the time the disease appeared in that island, horses were being transported into Havana from America.

b. The few places that were exempt from the disease were peculiarly situated, and, by reason of their situation, the importation of horses or mules was either impossible or improbable. To cite an example, during the prevalence of the malady, intercourse with Prince Edward Island was interrupted by the ice, with which, during the winter months, it is hemmed in, while in Vancouver's Island a quarantine was instituted against horses and mules. *Both of these islands were exempt.* Again, if we refer to the three or four towns on the continent in which the disease did not appear, such as La Paz, in Lower California, or Minatitlan, Tabasco and Merida, in Mexico, it will invariably be found that these places were all peculiarly situated, either at the extremity of a peninsula, or else in some remote and inaccessible region, where they were practically cut off from direct communication with towns in which the disease prevailed.

c. Additional logical proof of this proposition is derived from the fact that the rate of progress of the disease seemed to depend upon the extent of population of the district, and the amount and facility of commercial intercourse. In the thickly populated sections of the Eastern States, and along the line of the Pacific Railway, its advance was rapid, whereas in the more thinly settled regions of the West its progress was proportionately slow. At one point, the Sierra Nevada Mountains, which, during the winter months, are impassable, acted as an insurmountable barrier to the course of the epizoötic, and it was only able to find its way to California by following the circuitous route of the mining districts lying between Carson City and Inyo.

The medical profession are under great obligations to Dr. Judson for the indefatigable zeal manifested by him in investigating the varied phenomena characterizing this epizoötic. The theories which he puts forth respecting its mode of transference will be found to conform to

the canons of Bacon's philosophy, and will bear a close comparison with the recorded phenomena. It is by philosophical investigations, of which this forms a felicitous example, that we are slowly acquiring some knowledge of those mysterious poisons by the agency of which the spread of pestilential diseases is effected; and having at length been enabled to recognize the peculiar conditions required by each specific virus to facilitate its development, the next advance in the evolution of medical science consists in the application of these distinctions to practice, by elucidating some general principles, by the knowledge of which some of the predisposing influences of this class of diseases may be removed, or, at least, seriously modified.

HOBSON'S CHOICE.—*The Lancet* of June 20, 1874, refers in the following witty way to the sanitary condition of Cambridge:—

"A ludicrous discussion took place last week in the Town Council at Cambridge. The late Mayor had presented to the Guildhall a portrait of Hobson, the celebrated letter carrier, who arranged the posts between Cambridge and London during the latter part of the reign of Charles II. Old Hobson was an original character; in vain his customers endeavored to make their own selection of beasts to carry them on their journey, the horse whose turn it was to go out was invariably harnessed; this horse or none, the stout old postmaster offered, whence 'Hobson's choice.' Now Hobson was a sanitarian, and had a great idea of keeping the town sweet and clean. For this purpose, he bequeathed certain moneys to provide for a flow of fresh water to run perpetually on both sides of the streets of Cambridge. Drains were not invented in those days, or, doubtless, he would have employed the water for adequately flushing them. The present Mayor has ordered the portrait of this worthy townsman to be removed from the Guildhall. Of course, certain members of the Council are highly indignant, and attack the conduct of the Mayor. This gentleman defends himself on purely æsthetic grounds, and declares the picture, as a work of art, is not worthy the Guildhall. But we do not hold this to be the true motive for the removal. Has not the face of this ancient sanitarian of late assumed a stern, a reproachful gaze, so that the Mayor does not care to have any more private interviews with it? If this be the case, Hobson's portrait will be stowed away in the lumber-room, without giving him a choice in the matter, till Cambridge is once more clean. Meantime, the townspeople will be deprived of the likeness of their most famous citizen."

A CASE of rupture of the vagina with protrusion of the intestine has been the subject of a coroner's inquest in England, lately. Mr. Peacock, who had charge of the case, had used forceps, although it appears that the rupture probably occurred before the use of the instruments. The intestines protruded, and were cut off by Mr. Peacock. Dr. Barnes, who gave evidence in favor of Mr. Peacock, although he allowed that he should not have removed the intestines in this case, nevertheless thought that their removal did not arise from gross ignorance, want of skill or inattention. That he could conceive a case in which their removal might be the only thing that could be done, and might prolong life for a few minutes.

Under these circumstances, the jury, after a short deliberation, returned the following verdict: "That the deceased died from the effect of an escape of intestine consequent upon spontaneous rupture of the vagina during her confinement, adding their opinion that no blame was attachable to Mr. Peacock.

Correspondence.

The Recussitation of Persons apparently Drowned.

Messrs. Editors. — The "Directions for Restoring Persons apparently Drowned," lately reprinted in the daily papers, must have attracted the attention of others as they did that of your correspondent C. E. B. Had it not been for a disinclination to act the ungracious part of criticizing so philanthropic an association as the Massachusetts Humane Society, I also should have sent my comments to the *Daily Advertiser's* editorial waste-basket.

It may well be questioned whether any advantage results from attempts at this sort of instruction; but, as the subject has been opened, I venture to offer for dissection the accompanying "Directions," which I was tempted to print and post at a sea-side village, three years ago, just after a death by drowning had occurred, when the general ignorance of what to do in such an accident had especially impressed itself on my mind.

My idea was to frame a set of Rules which an average intelligence might comprehend; not to be appealed to at the moment of necessity, but which, by being conspicuously posted, in the depot, the store, or the post-office, should, by repeated perusal, penetrate the memory and understanding of the reader.

The "Marshall Hall" and the "Sylvester" methods cannot be explained without diagrams, and are not, even then, understood well enough to be intelligently practised except under the guidance of an expert. They are, I am informed, omitted from the latest "Directions of the British Royal Humane Society."

Perhaps the Rules which follow would better serve their purpose if all but numbers I., V., VI. and IX. were omitted. R. M. H.

DIRECTIONS FOR RESTORING PERSONS APPARENTLY DEAD FROM DROWNING.

I. Lose no time. Carry out these directions on the spot.

II. Remove the froth and mucus from the mouth and nostrils.

III. Hold the body, *for a few seconds only*, with the head hanging down, so that the water may run out of the lungs and windpipe.

IV. Loosen all tight articles of clothing about the neck and chest.

V. See that the tongue is pulled forward if it falls back into the throat. By taking hold of it with a handkerchief it will not slip.

VI. If the breathing has ceased, or nearly so, it must be stimulated by pressure of the chest with the hands, in imitation of the natural breathing; forcibly expelling the air from the lungs and allowing it to re-enter and expand them by the elasticity of the ribs. Remember that *this is the most important step of all.*

To do it readily, lay the person on his back, with a cushion, pillow, or some firm substance, under the shoulders; then press with the flat of the hands *over the lower part of the breast bone and the upper part of the abdomen*, keeping up a regular repetition and relaxation of pressure twenty or thirty times a minute. A pressure of thirty pounds may be applied with safety to a grown person.

VII. Rub the limbs with the hands, or with dry cloths, constantly, to aid the circulation and keep the body warm.

VIII. As soon as the person can swallow, give a table-spoonful of spirits in hot water, or some warm tea or coffee.

IX. Work deliberately. Do not give up too quickly. Success has rewarded the efforts of *hours*.

The Medical Education of Women.

Messrs. Editors,—I have just received, from my friend Mr. Lawson Tait, of Birmingham, England, a paper on the "Medical Education of Women," from which I extract several paragraphs giving a trustworthy explanation of the scandal that has been promulgated about some of the female medical students in the University of Zürich. This account clears up the

apparent discrepancy between current rumor and the protestations of Prof. Frey and others. The veritable students of medicine seem to be exonerated from all participation in the objectionable actions which evoked the Russian decree.

I trust you will allow me, by publishing this recital, to make *amende honorable* to the fair sex for crediting and maintaining, in a former communication, charges which now prove to be unmerited.

I am, very truly yours, JAMES R. CHADWICK, M.D.

123 *Boylston Street, July* 17, 1874.

"Zurich, like all free Swiss and English towns, is liable at any time to be the centre of some body of political refugees and conspirators, and for some months anterior to the invasion of the town by the lady students, a colony of Russians had established themselves there. What their object was I do not know, but the proximity of Rappershewyl schloss, the only property of the Polish nation, or rather of the Polish exiles, may account for their selection of Zurich as a resting place. Their numbers, at least, grew very rapidly, and after the English and German ladies, and a few Russian women who really were students, had become fairly installed at the University, they were reinforced by a number of young men and women from Russia who enrolled themselves as students of medicine, but who excited the ill-will of both professors and students by rendering it very evident that study was not their intention. So much were the earnest students affected by them that they instituted proceedings to have them, especially the women, removed, but desisted on the appeal of Prof. Meyer to the effect that if the University were to be closed to the women they did not like, it must also be closed to those whom they respected.

"The attention of the Russian ambassador at Berne seems to have been drawn to the Russian Colony at Zurich, and he made some representations to his government, which were brought to a crisis by one of the female pseudo-students being caught on the Russian frontier with some treasonable documents in her possession. The ukase was accordingly issued, and was directed only against the women, because there was no evidence against the men, and no ostensible reason for interfering with them, but also because the authorities at St. Petersburg well knew that where the women went the men would follow, and the result was as was expected, for Zurich was cleared of a lot of people whose presence neither the University authorities, nor the students, nor the people of the town at all approved of. These Russians, it is true, committed no offence that could be laid hold of, but the women were loud in their manner, smoked in the streets, and, to use the quaint words of one of the professors, 'they lived with their compatriots in houses which had too few rooms.' There are still left at Zurich six Russian ladies who are studying medicine earnestly, and against whose conduct and character not a word has been said; but the general effect of the ukase has been very disastrous on all, for female medical students have thereby been branded with an undefined charge of immorality, based on a substratum of truth, and of which it is not easy for them or for any one to disabuse the public mind. Of the women who were driven from Zurich by the ukase, a small number now study at Berne, two or three tried Leipzic, but failed to pass the matriculation examination which is necessary for entrance into that University, and some are in Paris."

Obituary.

AT a recent meeting of the Hampshire District Medical Society, tributes of respect were paid to deceased members: Drs. Alonzo Lewis, of Hatfield, who died of tubercular disease, the result of continued hard work in the profession, and E. M. Johnson, a victim of the Mill River disaster.

Medical Miscellany.

A PENALTY of £20 and costs (amounting to £5. 1s. 6d.) has been recovered through the Halifax County Court from a person named Anderson, a homœopathic chemist in Halifax, for illegally practising as an apothecary.—*The Lancet.*

THE LONDON HOSPITAL SUNDAY FUND.—The latest accounts state that the sum received thus far at the Mansion-house exceeds the entire result of last year's appeal, about £28,000 having been paid in. A much larger sum, however, was expected this, the second, year of the movement.

THE celebrated Mr. Thomas Banting left £20,000 for building a convalescent home at Worthing, to be called the Banting Memorial. When are we to have convalescent homes connected with our Boston Hospitals?

FRECKLES.—It is said that powdered nitre moistened with water, applied to the face night and morning, will soon remove freckles.—*Practitioner and Druggist*, May, 1874.

DR. U. V. WILLIAMS, of Bridgeport, Ky., reports having recently attended a colored girl, aged 11 years 3 months, who gave birth to a male child, well-formed, and weighing eight pounds. Mother and child did well.—*The American Medical Weekly.*

PROF. SCHIFF, of Florence, "has demonstrated the superiority of ether to chloroform, on the ground of safety to human life . . . that ether is unattended by any hurtful result, unless through gross carelessness . . . is so convinced of this as to affirm that every case of death resulting from the application of ether is wholly due to the incompetency of the physician by whom it may have been administered."

THE BRITISH MEDICAL ASSOCIATION will hold its annual meeting at Norwich on August 11th, 12th, 13th and 14th, the president being still Sir Wm. Fergusson, and the president elect Edward Copeland, M.D., Senior Physician to the Norfolk and Norwich Hospital. An address in medicine will be given by J. Russell Reynolds, M.D., Physician to the University College Hospital, and an address in surgery will be given by W. Cadge, Esq., Surgeon to the Norfolk and Norwich Hospital. An address will also be given by James Matthews Duncan, M.D., Lecturer on Midwifery and the Diseases of Women and Children in the School of Medicine, Edinburgh.

CYSTIC TUMOR ATTACHED TO THE KIDNEY.—At a recent meeting of the Medico-Chirurgical Society of Edinburgh, a case of the above, simulating ovarian disease, was reported. The kidney was extirpated, and the patient recovered. Mrs. S., 49, mill-worker, widow, noticed, eighteen months ago, a swelling in the lower iliac fossa, which made rapid progress. When admitted to the hospital, it was freely movable, and larger than a man's head. Three fluctuating points, which apparently communicated, were felt. Uterus was high in the pelvis. Ovariotomy was determined on. The peritoneum being opened, the cyst was exposed. It was tapped by a large trocar, but no fluid came. It was found to be of the consistence of porridge; the cyst was opened and two pints of stuff cleared out. Ovaries were found perfectly healthy, and in normal situation. Tumor was a cystic one, involving left kidney, which was removed after adhesions to a portion of bowel and omentum were broken up, and vessels and arteries tied. The patient made a tedious, but complete recovery, during which she passed about forty ounces, daily, of healthy, non-albuminous urine. It was remarked that the diagnosis was evidently an erroneous one. The tumor had grown in a very peculiar way, covering the adherent bowels, which were behind the tumor, instead of, as is usual in such cases, in front of the kidney.—*Edinburgh Medical Journal.*

DR. E. S. GAILLARD, who for many years has edited *The Richmond and Louisville Medical Journal*, has lately issued a weekly journal entitled *The American Medical Weekly*. "It will," the editor says, "be an independent medical journal, devoted not only to the medical but professional interests of American physicians, and to the protection and development of those interests which are justly confided to their charge." We wish the enterprising editor success.

PROF. HUGHES BENNETT, owing to a continuance of ill health, has resigned the chair of the Institutes of Medicine, which he has held now for twenty-six years. Already there are several candidates in the field. Among the names mentioned are Dr. John Cleland, Professor of Anatomy in Queen's College, Galway; Dr. J. B. Pettigrew, Pathologist to the Edinburgh Infirmary; Dr. J. G. McKendrick, former assistant to Dr. Bennett and others.

NOTES AND QUERIES.

"AMERICAN MODESTY."

UNDER the above title, the *London Medical Times and Gazette*, of July 11th, somewhat sarcastically says:—"The following richly modest remarks we extract from the seventh report of St. John's Hospital, Lowell, Massachusetts: 'There is this remarkable fact, that scarcely a patient enters the hospital but what improves day by day, seldom if ever getting any relapse, whereas in private practice [such] patients very frequently get worse by taking cold, by improper diet, by overdosing, or some other neglect or imprudence. This hospital affords a grand opportunity to test the merits of good nursing and the application in sickness of sanitary laws; the effect of these agencies may be seen immediately, and in many cases, seemingly to do the *cure*, with but little medicine.'"

The Report is treating of a certain class of inhabitants, numerous in every manufacturing place, especially so in Lowell, our great city of spindles. But general as the quoted remark may appear, dissevered of its qualifying clauses, it nevertheless contains a truth too often lost sight of or ignored here and in Great Britain. Too long have drugs been considered *the essential* in the treatment of the sick. We, Americans, have been generations in learning that the polypharmacy introduced from Old England by our forefathers has been, and still continues to be, one of the greatest obstacles to the advancement of medical science, and the just appreciation of the profession by the people. In the name of humanity, Brother Bull, avail yourself of every opportunity to witness disease in its natural course, *cured* without drugs, and refrain from sneering at honest confessions of their secondary importance. Thus may you find, perhaps, that your philosophy, though dreamy, is not all-comprehending.

MASSACHUSETTENSIS.

DIED,—In Lawrence, July 21st, Dr. Edmund Seyffarth.—At Hillsdale, Ill., July 16th, Calvin P. Fiske, M.D., formerly of Fiskdale, Mass., 68 years.

MORTALITY IN MASSACHUSETTS.—*Deaths in sixteen Cities for the week ending July* 18, 1874.

Boston, 132; Worcester, 12; Lowell, 22; Milford, 3; Chelsea, 5; Cambridge, 22; Salem, 8; Lawrence, 14; Springfield, 17; Gloucester, 8; Fitchburg, 3; Newburyport, 3; Somerville, 4; Fall River, 19; Haverhill, 4; Holyoke, 14. Total, 290.

Prevalent Diseases.—Consumption, 49; cholera infantum, 32; pneumonia, 14.

F. W. DRAPER, M.D.
Secretary pro tem. of the State Board of Health.

DEATHS IN BOSTON for the week ending Saturday, July 25th, 157. Males, 85; females, 72. Accident, 4; apoplexy, 1; inflammation of bowels, 2; bronchitis, 2; inflammation of the brain, 1; congestion of the brain, 4; disease of the brain, 4; cancer, 3; cholera infantum, 32; cholera morbus, 1; consumption, 23; convulsions, 3; croup, 1; cyanosis, 1; debility, 2; diarrhœa, 4; dropsy, 1; dropsy of the brain, 2; drowned, 3; dysentery, 1; diphtheria, 1; "fever," 1; scarlet fever, 2; typhoid fever, 7; disease of the heart, 5; intemperance, 1; jaundice, 1; disease of the kidneys, 3; disease of the liver, 3; congestion of the lungs, 4; inflammation of the lungs, 7; laryngitis, 1; marasmus, 7; measles, 1; old age, 4; paralysis, 1; pleurisy, 1; premature birth, 1; peritonitis, 1; pyæmia, 1; puerperal disease, 2; rheumatism, 1; suicide, 1; disease of the stomach, 1; thrush, 1; tetanus, 1; whooping cough, 2.

Under 5 years of age, 79; between 5 and 20 years, 11; between 20 and 40 years, 29; between 40 and 60 years, 19; over 60 years, 19. Born in the United States, 107; Ireland, 36; other places, 14.